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ADDRESS

Delivered by the President, Professor H. H. Turner, on presenting the Gold Medal of the Society to Professor Lewis Boss.

THE Gold Medal of the Society has been awarded to Professor LEWIS BOSS "for his long-continued work on the positions and proper motions of fundamental stars," and it is now my pleasant duty to lay before you the grounds on which the Council have made this award.

If, as we hope, our medals have been bestowed in recognition of the best work of the time, our list of medallists should be an epitome of the history of astronomy since the foundation of the Society in 1820. It is therefore satisfactory to find that one historical fact which is of importance in considering the work of Professor Boss is clearly brought out by this list—viz. that the need for observations of places of the fixed stars which characterised the earlier years has been gradually replaced by the need for careful discussion of the observations made. In 1820 our knowledge of the places of the stars rested chiefly on the work of Bradley, which was more than half a century old; so that the effects of precession and of proper motion, the former of which was only approximately known and the latter scarcely at all, were serious. More modern observations were sadly needed, and accordingly we find that the medal often went in those early years to the compilers of catalogues of stars—to Brisbane for observations at Paramatta, to Bessel for his zone observations, to Johnson for his Catalogue of 606 southern stars, to Wrottesley for his Catalogue of the R.A.'s of 1318 stars, to Argelander, to Rumker, and to Carrington. Professor Boss has more than once remarked that "the period 1820–1850 is richer than any other in the history of astronomy in fundamental determinations of a high class" (*A. J.* No. 423, p. 116).

But as observations accumulated the merits of such work were not ranked so high. Carrington was the last to receive, in 1859, a medal for a catalogue of stars of the fundamental kind; and already it had been practically recognised that the second historical period in fundamental astronomy had commenced. By comparing their observations with those of Bradley, astronomers determined the movements of the stars, not only the individual

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proper motions, but the general apparent motion which we call precession, and ultimately the systematic drift due to the motion of the solar system through space. Thus in 1850 Otto von Struve received the medal for his paper on the constant of precession; in 1852 we find the name of C. A. F. Peters, with a reference to his determination of nutation; and in 1858, in awarding the medal to the Rev. R. Main, the President of the day specially referred to the proper motion of the solar system, which, Mr. Main conceived, "ought to be admitted among the established facts of astronomy."

As one catalogue after another was compared for purposes of this kind with Bradley's observations it became more and more important to make sure that these observations, which were adopted by universal consent as the starting-point of all such inquiries, were reduced in the best possible way; and accordingly we find from our list that in 1888, so recently as to be within the memory of many here present, our medal was awarded to Professor Arthur Auwers "for his re-reduction of Bradley's observations."

The present award reminds us that the second period has in its turn been succeeded by a third, which discards Bradley's observations as the starting-point. The name and fame of Bradley are dear to us not merely because he was an Englishman, and represented England as Astronomer-Royal, but because of the greatness of the man himself. But however loth we may be to leave the old tradition which is rooted in Bradley we must recognise that we are come to the parting of the ways. Our medallist of to-day struck out the new path in his first great work a quarter of a century ago, and has trodden it steadily ever since. Dr. Auwers, who paid the tribute to Bradley of re-reducing his observations in order that they might yield their utmost, and who has plodded steadily and loyally along the old highway to the very last, has recently * retraced his steps to the turning, and taken the new road along which we all must travel for the future.

I should pay a poor compliment to our medallist if I allowed a possibility of existence to the misconception that the modern views involve any disparagement of Bradley himself. He would, I am sure, defer to no one in his admiration for Bradley's greatness, and for the splendid results which he obtained from the instruments of that day. It is the instruments alone that are in question, or rather about which there is no question. They were certainly inferior to those available in 1820, and it appears that the inferiority was so great that it is now preferable to use the observations of 1820 as a starting-point, in spite of

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* In his most recent rectification of the Fundamental Catalogue of the Berlin *Jahrbuch*, under date 1903 November 1, Dr. Auwers bases the declinations on fifty series *with epochs from* 1821 (*Bessel*) *to* 1897 (*Munich and Ottakring*). See *Ast. Nach.* No. 3927, p. 231, paragraph 3.

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Bradley's great skill as an observer and his greater antiquity.* Parallel situations have arisen in other departments of astronomy. For instance, until recently it was considered that the mean motion of the Moon and its changes could be best evaluated by starting from the naked-eye observations of ancient eclipses and comparing these with modern observations. The time elapsed since the invention of the telescope has made more accurate observations possible is too short—so it was considered—to give us information which would compare in value with that furnished by the much longer interval. But recently we have found reason to doubt the soundness of this view. The ancient observations are affected by uncertainties of historic record, and it seems even possible that modern observations can be used rather to correct the old records and identifications than that these can add anything to what can be obtained from comparatively recent work. And so in the case of Bradley's observations, which stood as the foundation of accurate work for so many years, it would now seem that modern observations can rather tell us what were the division errors of Bradley's circle than that we can learn any new facts by retaining the results obtained from this imperfect circle as a starting-point.

Such, as I have stated, was the view propounded with great courage and ability by Professor Boss in 1877 or earlier. I do not know what was his age at that time, but to judge from his present vigour he must have been a young man; and we have confirmatory evidence of this in the fact that he was only the Assistant-Astronomer attached to the United States Northern Boundary Commission. The great work which has become classical under the name of *Boss's Declinations* appeared in the comparatively humble guise of Appendix H to the Report of this Commission, prefaced by a brief letter from the Assistant-

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* Perhaps the following extracts from Professor Boss's writings of a later date put his position in a clear light. The italics are mine :—

"... No one would now think the instruments which Bradley used at all suited to the delicate task of precise determinations of star-positions. Observations made with them at the present time would be so distrusted for the possibility of anomalous systematic error that it is doubtful if they would be given any weight in comparison with the best contemporary observations. The distance in time and the isolation of Bradley's observations give them a real value, but their actual precision then is no greater than it would be now.

"... The trouble is, that for observations like those which were made with the rude instruments of past times *even by the greatest of observers*, it is impossible to arrive at more than a rough approximation to the systematic errors which they contain (*Ast. Jour.* No. 423, p. 115, 1898 January 3).

"... The object-glass of Bradley's transit was not achromatic, and the effective aperture employed was only 1½ inch. Only one (coarse) transit wire was usually observed, and thus it requires no vivid imagination to call up apprehensions even more grave and forbidding than those which Professor Newcomb has entertained in regard to the older observations of faint stars. ...

"... Bradley observed with his transit fully exposed to the Sun's rays: and there was not much improvement in this respect until long after his time. ... His transit weighed only 55 pounds, and was 8 feet long with an axis 4½ feet in length" (*Ast. Jour.* No. 430, p. 171).

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Astronomer to his chief apologising for the "unexpected delay" in transmission.

In the Introduction following this letter it is explained that the latitudes of twenty-two stations in the vicinity of the 49th parallel, which separates the United States from Canada, depend on two factors—the accuracy of the observations made and the correctness of the declinations adopted: that the former might be considered beyond reproach, and that it therefore became necessary to examine the latter. To make the declinations as trustworthy as possible all the authorities which could be obtained from the library of the United States Naval Observatory had been consulted; that a comparison of these various catalogues *inter se* was naturally suggested; that the scope of the work gradually enlarged, until the final outcome was the volume in question, containing an exhaustive discussion of nearly one hundred fundamental catalogues, tables for the systematic correction of them all, and a final catalogue of the declinations and proper motions of 500 stars for the epoch 1875, which at once took a place in the very front rank. To emphasise the special characteristic of this work to which I have already called attention I will quote the following paragraph from the "preliminary statement:"—

> It will be shown that the interval of time between the group of early determinations by Bessel (1821), Struve (1824), and Argelander (1829), and the later ones at Leiden, Melbourne, Greenwich, and Washington Observatories (not to mention intermediate catalogues), is quite sufficient for an independent judgment as to the approximate accuracy and consequent weight of Bradley's results, and that a reliable system of corrections to the various catalogues may be founded on a discussion of recent catalogues alone, taking as the earliest that of Bessel for the mean epoch 1821 (p. [7] or 413).

We have been led to believe that our cousins in the United States regard their boundaries somewhat seriously. There is a well-known story, scarcely, however, suitable for repetition from this Chair, which assigns as one boundary the North Pole, and as another the Day of Judgment. But I venture to think that even these ideals, after making due allowance for the national love for picturesqueness of statement, scarcely surpass in magnificence the conception of duty which led a young assistant astronomer, in the ordinary course of his work on a Boundary Commission, to undertake, as Appendix H to the Report, the collation and revision of all existing catalogues, and to initiate a totally new departure in principle which the world has now adopted some quarter of a century later.

In an Address like the present it is impossible to follow this work in detail; but the brief description already given is doubtless sufficient to convey an idea of the colossal labour involved and of the energy of the man who could carry it through. Before passing on to his other work, however, I will add, by way of emphasis, his own description of his attitude at the

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conclusion of his exertions. He proceeded by the method of successive approximations, which involved going over the whole material at least twice; and so little was his ardour damped by this repetition that he says at the end of it :—

> It would have been for me a pleasant task to have undertaken, with the help of the places now available, a third approximation to the systematic corrections and weights. But the real object of the work has been already sufficiently accomplished, and the time is not at my disposal for the purpose (p. [164] or 570).

He had in fact been appointed, during the course of the work, Director of the Dudley Observatory at Albany, a position which he still holds, and which he has made famous. We can readily understand how, in the early years of such an appointment, his attention was claimed by matters more directly relating to the Observatory, and especially by the Albany zone observations (+0° 50′ to +5° 10′), which were undertaken according to the plan of the *Astronomische Gesellschaft.* This work was commenced in 1878 August, immediately after the return of the Director from observation of the total solar eclipse of that year, and claimed his attention for nearly a dozen years in all. Considering that during this time Professor Boss took charge of a transit of *Venus* expedition, and executed many other pieces of work; that he seldom had more than one assistant of any kind, and sometimes none at all; and that the zone observations were made and reduced in no mere perfunctory or superficial manner, but with the thoroughness characteristic of all his work, we may well marvel that it was completed so soon.

In illustration of this thoroughness it may be cited that in 1879 and 1880 the division correction required by each of the two circles employed for the observations, as well as the flexure of each, was investigated "by extremely laborious series of observations," although the knowledge thus obtained was not actually required in carrying out the strict programme of the *Astronomische Gesellschaft*, which demanded differential observations only. Well-marked flexures were found for both the circles, amounting to about a second of arc at maximum. The magnitude-equation was also carefully determined by observations with gauze screens; and, as we might confidently expect in Professor Boss's work, all available catalogues were compared with his observations, and a most valuable series of proper motions was deduced for the stars in this zone.

From a discussion of these he was led to the unexpected result that the effect of the Sun's motion through space is well marked among stars of the ninth magnitude, though it might naturally have been supposed from their faintness that these stars were too far away to show this perspective effect. Following up this clue, Professor Boss made two determinations of the solar motion from the slender material afforded by his observations—one from stars of mean magnitude 6·6 and another from

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those two magnitudes fainter. He found that the Sun's apparent angular velocity came out nearly the same for both series, and was thus led to the startling conclusion that for the stars considered "the true criterion for estimating their average distances is very nearly independent of the magnitude, and that it is almost wholly some function of the apparent proper motion." This fundamental fact, obtained from the discussion of observations necessarily restricted to a narrow belt of the heavens, was soon afterwards * confirmed by Dr. Oscar Stumpe, using independent material applicable to the whole sky.

On the completion of the Albany zone, or perhaps even before it was finished, Professor Boss began to do what he could for the Southern Hemisphere by observing stars in the region —20° to —40°. It is among astronomers a well-known and deeply regretted fact that owing to the accumulation of observatories almost exclusively in the Northern Hemisphere our knowledge of the southern sky is far behind that of the northern. Professor Boss was director of one of the numerous northern observatories, but he reached out southwards as far as possible, and did what he could to lessen the disparity. Moreover, though restricted as regards observation he was free as regards discussion; and he brought the same skill to bear on the collation of southern catalogues that he had formerly displayed for northern. In 1898 he published a systematic comparison of seventeen southern catalogues and a standard system of places and proper motions of 179 southern stars (*A. J.* Nos. 448–450). Three years later he was able to include a few more catalogues, and to deduce from southern stars a determination of the solar motion in space, which afforded satisfactory independent confirmation of the results found from northern stars, and brought out several minor features of interest (*A. J.* 499–501).

Meanwhile the Dudley Observatory had been, in 1893, transferred to a new site. The old Observatory was near a great railroad which skirted the hill at a distance of about 160 yards, and was in need of extensive repair. Miss C. W. Bruce, of New York, who has been a liberal benefactor to astronomy in so many directions, offered $25,000 additional endowment for the institution on condition that the "friends and neighbours" of the Observatory should secure its removal to a new and better site. This condition was promptly met by the subscription of another $25,000 by sixty-five individuals, nearly all of whom were residents of Albany; and the city itself gave land for the new site and $15,000 in exchange for the grounds and buildings of the old Observatory. Miss Bruce thereupon increased her donations to $35,000. Professor Boss thus found himself in a greatly improved position. He was able to superintend personally

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* The conclusions of Professor Boss were published in the *Astronomical Journal* for 1890 March 14. Dr. Stumpe's research is dated 1890 June and is published in *Astronomische Nachrichten*, Nos. 2999–3000, under date 1890 October 21: it contains no reference to Professor Boss's paper.

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the re-erection of his transit circle at the new Observatory, and took special pains with the foundations. The observations of southern stars, temporarily interrupted, were soon resumed; and the Pruyn Equatorial of 12 inches aperture was added to the Observatory equipment by the liberality of the sons of the late Robert H. Pruyn, formerly president of the Observatory Trustees.

But an important question arose in consequence of the removal. Could the meridian circle be regarded as essentially the same instrument as before? or had the inevitable small jars of travel produced alterations, too slight perhaps to be noticed by the eye, but serious for astronomical observations? One test could be applied which might afford at any rate a partial answer to these questions. We have seen that the division errors and flexures of the two circles were carefully determined at the old site: they might be re-determined after the re-erection for comparison with the former values. It is true that the labour involved was enormous, and might well have deterred a less resolute man than our medallist. To him it was a mere circumstance, and he entered upon the "fourteen months' convict labour for four persons"—as I have heard him describe it—without a thought of evading any portion of it. Rather did he improve the occasion by pushing the research further than he had done before. He was rewarded by the attainment of several conclusions of a far-reaching character; as, for instance, that larger circles than those usually provided might profitably be used in meridian observing (*A. J.* No. 383); that systematic errors as opposed to individual corrections are an important part of the total error of graduation, especially when the subdivisions are made by use of a scale; and that the determination of the individual corrections may indeed serve no useful purpose (*A. J.* No. 401).

But the most important of all was the gratifying discovery that neither the division errors nor the flexures had been sensibly changed by the journey from the new site to the old; for not only did this fact bear eloquent testimony to the accuracy and sufficiency of both investigations; not only did it render plausible the inference that the whole instrument might be regarded as unaltered, so that the observations might be resumed without fear of discontinuity; but it suggested the important corollary that a longer journey such as that to the Southern Hemisphere might be undertaken with equal probability of preserving continuity; for the jars met with on a long sea voyage need not be so serious as those encountered during the transport over a few miles of road. To this conclusion I shall recur later, as it has a special importance with regard to Professor Boss's views of the consolidation of stellar positions.

We have seen that he began with the discussion of northern stars and proceeded to deal with southern. Neither discussion, however, was altogether exclusive; that of northern stars was not complete without some reference to southern catalogues, and

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vice versa. But gradually he was led to a general scheme of consolidation for all catalogues, both north and south, which is still engaging his attention, though he has already completed the first and most important stage, and published the results in a series of memoirs of which I shall presently speak.

But before doing so I would call your attention to the steadiness of purpose which runs through all the work of our medallist. The terms of the Council award, "his *long-continued* work on the positions and proper motions of fundamental stars," are particularly appropriate, for the work has been continued throughout his career as an astronomer. His appointment to the Dudley Observatory hastened the close of his discussion of the northern declinations, but only initiated his attack on the main problem from a new point of view. The restriction to zone observations, which might have swept the notion of fundamental discussion out of the thoughts of another, did not deter Professor Boss from deducing the constant of precession and the elements of solar motion in space from these same zone observations.

But it must not be supposed that he neglected all other work. The pages of the *Astronomical Journal* contain numerous papers by him relating to comets, both observations and ephemerides. I will venture to quote from one of these papers a few words which seem to me to indicate the characteristically wide sweep of his outlook :—

> It had occurred to me [he says] that the theory of the September comet of 1882 possesses uncommon interest from more than one point of view, and that this interest may become very intense and extremely inquisitive during the twenty-seventh century, when the fragments of the comet shall successively make their next appearance at perihelion, and when the positions of comparison stars may be investigated with a degree of rigour unattainable at the present time (*A. J.* No. 226, p. 75).

Accordingly, he gives the positions of 465 comparison stars determined at Albany; and I will hazard the prophecy that when the astronomers of the twenty-seventh century come to investigate them they will give them the maximum weight for the epoch.

But he rightly judged that he could be of best service to astronomy by using the fruits of his ripe experience in the line of work he had adopted from the first, and that such work as could be done equally well by others should be left to others; and this position he has consistently maintained in reply to inquiries from those who found it difficult to understand the importance of fundamental discussions, and who looked rather for tangible discoveries. Thus it is related of him that when a member of his Board of Trustees confessed to some disappointment that no comets were discovered at the Dudley Observatory he explained what a drawback it would be to neglect his cherished work for such a purpose, but declared that he could

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almost certainly discover a comet vicariously for them if they would supply a modest sum, which he named, for payment of an assistant. And this promise he was actually able to perform! The money was promptly subscribed; the assistant was engaged and instructed what to do and how to do it, and within a short time the notable Comet 1882 (*a*) was found. It was a bold stroke, but Professor Boss was playing for a big stake. He wished to obtain freedom to work in the way he believed to be most important for astronomy, while retaining the confidence and approval of his trustees; and by taking a risk which he felt was not unreasonable he managed to secure this happy combination. We know at any rate that he has produced no lack of work after his own heart; and that he has the cordial support of the board of trustees and the citizens of Albany generally we know from the ready way in which they subscribed the money requisite for the removal and renovation of the Observatory in 1893.

But we must now turn to the work which is occupying the attention of our medallist at the present time, and of which an important section has been recently published in a series of papers, originally printed in the *Astronomical Journal* and afterwards collected under the title "A Catalogue of 627 Principal Standard Stars distributed from the North to the South Pole, and derived through a Homogeneous Treatment of all available Observations," by Lewis Boss (Albany, N.Y., 1904). This work is to be regarded, the author tells us, as the exhibit of an intermediate stage of an investigation which is in progress, and which cannot be completed until further important series of observations, *especially in the Southern Hemisphere*, provide the basis for a more definitive computation. Indeed, it is characteristic of such work that it can never be completed in a final sense. The addition of new observations will always necessitate their incorporation with the old, and consequent slight modifications. But as the mass of material already discussed increases in bulk longer series of new observations will be required if any sensible impression is to be produced by their addition, and hence longer intervals of time will naturally elapse between revisions. Had our knowledge of the Southern Hemisphere been more complete the present discussion might be confidently expected to suffice for some considerable time.

The work is presented in six sections. The first gives the catalogue of places and proper motions, with comparisons with the Catalogues of Auwers and Newcomb; the second and third describe the method of forming the right ascensions, with a discussion of magnitude equation; the fourth describes the corresponding method of forming declinations; the fifth discusses the declinations of Bradley in the light of these modern observations; and the sixth gives general tables of weights and systematic corrections and notes relating to various catalogues, of which over seventy were completely discussed. Instead of attempting to deal with such a comprehensive work as a whole,

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I shall content myself with special references to two points of general interest which will illustrate the character of Professor Boss's methods and the fundamental nature of the results obtained.

The first is taken from his discussion of magnitude equation. For the benefit of those whose work lies in directions other than fundamental astronomy I may briefly recall the fact that this refinement of what has long been known as personal equation was definitely* discovered by Sir David Gill in his discussion of the comparison stars for *Mars* in 1878, and has since that time occupied an ever-increasing share of the attention of astronomers. He found that when the observations made by two different observers were compared they differed by an amount which varied with the brightness or magnitude of the star compared. Hence the name "magnitude equation." As in the case of the older "personal" equation, this kind of error was first noticed in connexion with transit observations; but it has been sought and found in other directions, even in photographic measures, though the origin of the discrepancy is there essentially different in character. It seems probable that magnitude-equation has taken up a permanent position as a factor—and a very troublesome factor—in all astronomical measurement of great refinement.

But we are concerned immediately with the particular form of it which occurs in making transit observations, and which consists in the fact that the observer, watching the passage of a faint star over a wire, tends to record a later instant than he would for a bright star. I have used the definite word "later" advisedly, for it has been ascertained that, while the amount of lag for one observer is different from that for another, there is a general consensus to be late for faint stars, or, if we prefer the statement, early for bright ones.

It has been ascertained by Professor Boss that the peculiarity for a given individual persists in spite of very deliberate changes in method of observing. It is well known to transit observers that they may adopt one or other of two mental processes in pressing the electric key to record a transit: they may commence pressing slightly before the star reaches the wire, so that the contact may occur as nearly as possible when the star is centrally on the wire, *or* they may wait until the star is on the wire and then begin to press. The difference may seem trivial to those who have not tried it in practice, but it represents a very real change in method to the observer. Professor Boss devoted some months to changing his personal habit from one plan to the other, and succeeded to his complete satisfaction, but found that nevertheless his magnitude-equation remained unchanged (*A. J.* No. 516, p. 100).

But a much greater revolution than this has been made in the

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* Argelander and Gould had suggested the probability of its existence, but these were mere conjectures.

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observation of transits during the last century. Before these days of electric apparatus, when we merely press a button, there were the old days of eye-and-ear observing—a completely different method. Was there a magnitude-equation then? and was it of the same kind as in these days? If not, then the proper motions of stars deduced from a comparison of ancient and modern observations will be systematically different according as the stars are bright or faint; and this might lead to erroneous conclusions, for instance, about the distance of the faint stars as compared with bright; in other words, about the structure of the universe, for in such ways does our knowledge of the vast in astronomy depend upon our care of the minute. We must find out, if we can, what was the average character of magnitude-equation in eye-and-ear transits compared with its average modern value; and Professor Boss's conclusion, from an exhaustive discussion of all these catalogues, that for all practical purposes this troublesome source of error has remained unchanged * by the revolution in method is not only of the first importance, but particularly welcome.

As a second illustration I take, not one of the results obtained by him, but one of his methods of work—viz. his method of combining a northern and a southern catalogue so as to obtain a knowledge of the refraction at each Observatory, and to free the observations accordingly from errors of adopted refraction. I do not know whether Professor Boss was the first (in 1877) to use this method, which is now familiar. He makes no claim to originality for it in his writings; rather does he say that "this method of deducing conclusions as to the most probable value of the refraction constant for each of the sets of observations compared has been recognised ever since the time when accurate observations were first made in the Southern Hemisphere." (*Catalogue of* 672 *Stars*, p. 63, or *A. J.* No. 540, p. 119.) But it may be worth recording that so eminent an investigator as Main, working well within the limits of time mentioned by Professor Boss, had not recognised the essential feature of the method; for in combining in this way obervations at Greenwich and the Cape he "assumed that the constant of refraction is not affected by locality; that it is the same at the Cape as it is at Greenwich" (see *Monthly Notices*, vol. xviii. p. 126).

Now this assumption may be justified as regards the theoretical constant of refraction; but Professor Boss has truly remarked that in practice we are concerned with a value of this constant modified by the particular barometer or thermometer in use, or the way in which the latter is exposed; and hence we must regard each Observatory as having its own special constant. This does not, however, prevent us from combining the Greenwich with the

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* *Catalogue of* 672 *Stars*, p. 55. It is of interest to remark that this conclusion was previously quite unexpected by Professor Boss himself, who had formed a strongly contrary opinion (see *A. J.* No. 423, p. 117, last paragraph).

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Cape observations; it only brings out the strength of the combination. Near the Greenwich zenith the Greenwich refractions would be sensibly the same, whatever reasonable value we assigned to the Greenwich constant, and the comparisons therefore leave us free for the evaluation of the Cape constant. Near the Cape zenith, similarly, the Cape refractions do not matter, and we are left free to determine those of Greenwich. The mutual accommodation is so complete that we are almost reminded of the classical instance of the united couple, one of whom could eat no lean, the other no fat. But there is one small flaw in the agreement. Refraction is not the only source of error in declination observations; there are instrumental errors as well; and these are necessarily entangled with the uncertainties of refraction. By heroic labour these instrumental errors may be ascertained; but there are not many men who, like our medallist, will cheerfully embark on "fourteen months' convict labour" to investigate them. A less satisfactory alternative is to trust to the general elimination of these individual errors in the mean of a number of catalogues made with different instruments; but we are here met by the difficulty that in the Southern Hemisphere the number of instruments is lamentably few, and in past years was fewer still; so that the total number of southern catalogues available for combination with northern on this beautiful plan is very small. Hence we are now in a position to gauge the merits of a scheme which I know lies very near the heart of our medallist, and which it is only necessary to mention in order to recognise its great importance for fundamental astronomy. We have seen that Professor Boss has determined the instrumental errors of his Olcott meridian-circle with the utmost care on two occasions, obtaining results which mutually confirm each other. Moreover, the instrument was transported bodily to some distance between the determinations without apparent disturbance, so that the possibility of moving it without injury has been demonstrated. Why, then, should it not be transported bodily to the Southern Hemisphere, and a southern catalogue made with it for comparison with observations already made in the Northern Hemisphere, which may possibly be supplemented by others on its return? In this way the uncertain factor in the combination of the two catalogues would be eliminated; the instrumental errors would be the same in both cases, and would, moreover, be accurately known. The observations would be entirely free for the determination of the two refraction constants, and a system of star-places would result of an excellence hitherto unparalleled.

This, as I have said, is the scheme which has gradually matured in Professor Boss's mind during a life of concentrated work steadily directed towards one goal, though as yet he cannot foresee with satisfactory probability the provision of means for putting it into execution.

It may well excite our wondering admiration to find that our medallist, after the long and arduous labours I have so inade-

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quately described to you, with work, too, in hand requiring unremitting attention at the present time, should be looking ahead, not to a period of well-earned leisure, but to a scheme of this kind which involves, not only considerable risk and anxiety, not only renewed assiduity such as a younger man might well shrink from, but exile from home for a number of years. There can be but one opinion as to the value of such an enterprise, and but one thought in the minds of all those anxious for the welfare of astronomy—an earnest hope that means and opportunity may be found for putting so noble a project into execution. We may be aware that we are thereby hoping that the work of our medallist, as it has been "long continued" in the past, shall be also long continued in the future; but at the same time we are aware that for such a man the hope is only another expression of a hope for his long-continued happiness.

At the beginning of this Address I referred to our list of medallists as in some sort an epitome of astronomical history during the past eighty-five years. One of the features of that history which gives us as Englishmen peculiar gratification, and which is faithfully reflected in this list, is the steady and rapid development of astronomical work of the first order in the United States. At the foundation of our Society it could scarcely be said to exist; at the present day its growth is so vigorous that for the third time within five years we have the privilege and pleasure of recognising the merits of an American astronomer. And I would remark upon the varied character of the work done, as exemplified in the last three awards to stellar photometry, to solar spectroscopic research, and to fundamental astronomy. We are in presence of no mere handful of rare exotics, all of precisely the same type and dependent for their existence on the provision of plentiful artificial stimulus, but of a healthy and vigorous garden. Not that artificial warmth, when it can be obtained, is for a moment to be disparaged. Last year we had an opportunity of admiring the magnificent work which had been fostered by the provision of a great telescope and a great Observatory; and we know otherwise how much of the advance of astronomy in the United States has been due to the munificence of private citizens. It is all the more satisfactory to recognise in the very existence of men like Chandler and Boss that work can also be done under very different conditions. Professor Boss received timely aid from Miss Bruce and from the citizens of Albany in the removal of his Observatory, but beyond this the greater part of his work has been accomplished with very modest financial means; which is a new reason for hoping that, now the time has come when he requires help of a kind which has so freely been accorded elsewhere, he may not be long in finding it.

The journey across the Atlantic at the present time of year is a serious one, and we hear without surprise, though with rather selfish regrets, that our medallists cannot interrupt their

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work to undertake it. In their absence it has been a great pleasure to us to be honoured by the genial presence of Mr. Choate; and it is particularly kind of him to-day to spare us a few moments from the busy weeks preceding a departure which the whole of England unites in deploring.

(The President then, addressing the American Ambassador, said:)

Mr. Choate, in sending this medal to Professor Lewis Boss will your Excellency kindly assure him of our great admiration for his long-continued work and our deep sense of its fundamental importance for astronomy, and our earnest hope that means may be found for carrying into effect his splendid plans for the future?

The Jackson-Gwilt Gift and Medal.

The Jackson-Gwilt Gift and Medal have been awarded to Mr. John Tebbutt, of Windsor, N.S.W., for his important observations of comets and double stars and his long-continued services to astronomy in Australia, extending over forty years.

This gift has only twice been awarded previously—in 1902 to Dr. Anderson, and in 1897 to Mr. Lewis Swift. In announcing the award in 1897 the President of the day stated that, after some consideration, he had decided not to give an address in connexion with the award, and I shall follow the precedent thus set. But there can be no reason why I should not recall to your memory that Mr. Tebbutt began astronomical work in 1854, and is only now relinquishing it at the age of seventy; that amid surroundings which gave him little encouragement he has made regular and systematic observations during half a century, including, for instance, those of some 1400 occultations of stars by the Moon and valuable measures of double stars; that he has contributed over eighty papers to the *Monthly Notices*; and has, moreover, discovered several comets, including the notable ones of 1861 and of 1881, the orbits of which he computed from his own observations.

In handing this medal to the Secretary for transmission, with the gift, to Mr. Tebbutt, I will ask him to convey our hearty congratulations to the recipient on the accomplishment of half a century of single-handed astronomical work for which it would be difficult, if not impossible, to find a parallel, and our delight that Mr. Tebbutt should be entering upon the rare enjoyment of a thoroughly well-earned period of rest.

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The meeting then proceeded to the election of Officers and Council for the ensuing year, when the following Fellows were elected :

President.

W. H. Maw, Esq.

Vice-Presidents.

Sir W. H. M. Christie, K.C.B., M.A., D.Sc., F.R.S., Astronomer-Royal.
Major P. A. MacMahon, D.Sc., F.R.S.
H. F. Newall, Esq., M.A., F.R S.
H. H. Turner, Esq., D.Sc., F.R.S., Savilian Professor of Astronomy, Oxford.

Treasurer.

Major E. H. Hills, C.M.G., R.E.

Secretaries.

Thomas Lewis, Esq.
E. T. Whittaker, Esq., M.A.

Foreign Secretary.

Sir William Huggins, K.C.B., O.M., LL.D., D.C.L., F.R.S.

Council.

Sir William Abney, K.C.B., R.E., D.C.L., F.R.S.
Sir R. S. Ball, M.A., LL.D., F.R.S., Lowndean Professor of Astronomy and Geometry, Cambridge.
Rev. A. L. Cortie, S.J.
P. H. Cowell, Esq., M.A.
F. W. Dyson, Esq., M.A., F.R.S.
Alfred Fowler, Esq., Assistant Professor of Physics, South Kensington.
J. W. L. Glaisher, Esq., M.A., Sc.D., F.R.S.
A. R. Hinks, Esq., M.A.
E. B. Knobel, Esq.
A. A. Rambaut, Esq., D.Sc., F.R.S., Radcliffe Observer.
S. A. Saunder, Esq., M.A.
E. J. Spitta, Esq.